18 inches from the surface has been proved to contain the nitrifying organism by the fact that it has produced nitrification in the solutions to which it was added; while in twelve distinct experiments made with soil from greater depths no nitrification has yet occurred, and we must therefore conclude that the nitrifying organism was not present in the samples of soil taken. The third series of experiments has continued as yet but three months and a half; at present no nitrification has occurred with soil taken below 9 inches from the surface. It would appear, therefore, that in a clay soil the nitrifying organism is confined to about 18 inches from the surface; it is most abundant in the first 6 inches. It is quite possible, however, that in the channels caused by worms, or by the roots of plants, the organism may occur at greater depths. In a sandy soil we should expect to find the organism at a lower level than in clay, but of this we have as yet no evidence. The facts here mentioned are in accordance with the microscopical observations made by Koch, who states that the micro-organisms in the soils he has investigated diminish rapidly in number with an increasing depth; and that at a depth of scarcely 1 metre the soil is almost entirely free from bacteria.

Some very practical conclusions may be drawn from the facts now stated. It appears that the oxidation of nitrogenous matter in soil will be confined to matter near the surface. The nitrates found in the subsoil and in subsoil drainage waters have really been produced in the upper layer of the soil, and have been carried down by diffusion, or by a descending column of water. Again, in arranging a filter-bed for the oxidation of sewage, it is obvious that, with a heavy soil lying in its natural state of consolidation, very little will be gained by making the filter-bed of considerable depth; while, if an artificial bed is to be constructed, it is clearly the top soil, rich in oxidising organisms, which should be exclusively employed.

The Substances susceptible of Nitrification.—The analyses of soils and drainage waters have taught us that the nitrogenous humic matter resulting from the decay of plants is nitrifiable; also that the various nitrogenous manures applied to land, as farmyard manure, bones, fish, blood, rape-cake, and ammonium salts, undergo nitrification in the soil. Illustrations of many of these facts from the results obtained in the experimental fields at Rothamsted, have been published by Sir J. B. Lawes, Dr. J. H. Gilbert, and myself, in a recent volume of the *Journal* of the Royal Agricultural Society of England. In the Rothamsted Laboratory, experiments have also been made on the nitrification of solutions of various substances. Besides solutions containing ammonium salts and urea, I have succeeded in nitrifying solutions of asparagine, milk, and rape-cake. Thus, besides ammonia, two amides, and two forms of albuminoids have been found susceptible of nitrification. In all cases in which amides or albuminoids were employed, the formation of ammonia preceded the production of nitric acid. Mr. C. F. A. Tuxen has already published in the present year two series of experiments on the formation of ammonia and nitric acids in soils to which bone-meal, fish-guano, or stable-manure had been applied; in all cases he found the formation of ammonia preceded the formation of nitric acid.

As ammonia is so readily nitrifiable, we may safely assert that every nitrogenous substance which yields ammonia when acted on by the organisms present in soil is also nitrifiable.

Certain Conditions having Great Influence on the Process of Nitrification.—If we suppose that a solution containing a nitrifiable substance is supplied with the nitrifying organism, and with the various food-constituents necessary for its growth and activity, the rapidity of nitrification will depend on a variety of circumstances:—(1) The degree of concentration of the solution is important. Nitrification always commences first in the weakest solution, and there is probably in the case of every solution a limit of concentration beyond which nitrification is impossible. (2) The temperature has great influence. Nitrification proceeds far more rapidly in summer than in winter. (3) The presence or absence of light is important. Nitrification is most rapid in darkness; and in the case of solutions, exposure to strong light may cause nitrification to cease altogether. (4) The presence of oxygen is of course essential. A thin layer of solution will nitrify sooner than a deep layer, owing to the larger proportion of oxygen available. The influence of depth of fluid is most conspicuous in the case of strong solutions. (5) The quantity of nitrifying organism present has also a marked effect. A solution seeded with a very small amount of organism will for a long time exhibit no nitrification, the organism being (unlike some other bacteria) of very slow growth. A solution receiving an abundant supply of the ferment will exhibit speedy nitrification, and strong solutions may by this means be successfully nitrified, which with small seedings would prove very refractory. The speedy nitrification which occurs in soil (far more speedy than in experiments in solutions under any conditions yet tried) is probably owing to the great mass of nitrifying organisms which soil contains, and to the thinness of the liquid layer which covers the soil particles. (6) The rapidity of nitrification also depends on the degree of alkalinity of the solution. Nitrification will not take place in an acid solution, it is essential that some base should be present with which the nitric acid may combine; when all available base is used up nitrification ceases. It appeared of interest to ascertain to what extent nitrification would proceed in a dilute solution of urine without the addition of any substance save the nitrifying ferment. As urea is converted into ammonium carbonate in the first stage of the action of the ferment, a supply of salifiable base would at first be present, but would gradually be consumed. The result of the experiment showed that only one-half the quantity of nitric acid was formed in the simple urine solution as in similar solutions containing calcium and sodium carbonate. The nitrification of the urine had evidently proceeded till the whole of the ammonium had been changed into ammonium nitrate, and the action had then ceased. This fact is of practical importance. Sewage will be thoroughly nitrified only when a sufficient supply of calcium carbonate, or some other base, is available. If, instead of calcium carbonate, a soluble alkaline salt is present, the quantity must be small, or nitrification will be seriously hindered. Sodium carbonate begins to have a retarding influence on the commencement of nitrification when its amount exceeds 300 milligrammes per litre, and up to the present time I have been unable to produce an effective nitrification in solutions containing 1·000 gramme per litre. Sodium hydrogen carbonate hinders far less the commencement of nitrification. Ammonium carbonate, when above a certain amount, also prevents the commencement of nitrification. The strongest solution in which nitrification has at present commenced contained ammonium carbonate equivalent to 368 milligrammes of nitrogen per litre. This hindrance of nitrification by the presence of an excess of ammonium carbonate effectually prevents the nitrification of strong solutions of urine, in which, as already mentioned, ammonium carbonate is the first product of fermentation. Far stronger solutions of ammonium chloride can be nitrified than of ammonium carbonate, if the solution of the former salt is supplied with calcium carbonate. Nitrification has in fact commenced in chloride of ammonium solutions containing more than 2 grammes of nitrogen per litre.

The details of the recent experiments, some of the results of which we have now described, will, it is hoped, shortly appear in the *Journal* of the Chemical Society of London.

Harpenden, July 21

RESEARCHES ON THE ORIGIN AND LIFE-HISTORIES OF THE LEAST AND LOWEST LIVING THINGS[1]

II.

BUT the point of difficulty was *B. termo*. The demonstration of its flagella was a task of difficulty which only patient purpose could conquer. But by the use of our new lenses, and special illumination we—my colleague and I—were enabled to demonstrate clearly a flagellum at each end of this least of living organisms, as you see, and by the rapid lashing of the fluid, alternately or together, with these flagella, the powerful, rapid, and graceful movements of this smallest known living thing are accomplished. Of course these fibres are inconceivably fine—indeed for this very reason it was desirable, if possible, to *measure* it, to discover its actual thickness. We all know that, both for the telescope and the microscope, beautiful apparatus are made for measuring minute magnified details. But unfortunately no instrument manufactured was delicate enough to measure *directly* this fibre. If it were measured it must be by an indirect process, which I accomplished thus:—The diameter of the body of *B. termo*, *i.e.* from side to side, may in different forms vary from the 20- to the 50-thousandth of an inch. *That* is a measurement which we may easily make directly with a micrometer. Having ascertained this, I determined to discover the ratio of thickness between the body of the Bacterium and its flagellum—that is to say, to discover how many of the flagella laid side by side would make up the width of the body.

---

[1] By Rev. W. H. Dallinger, LL.D., F.R.S., F.L.S., Pres. R. M. S. Continued from p. 622.

I proceeded thus. This is a complicated microscope placed on a tripod, so arranged that it may be conveniently worked upright. There is a special instrument for centering and illuminating. On the stage of the instrument, the Bacterium with its flagellum in distinct focus is placed. Instead of the simple eyepiece a *camera lucida* is placed upon it. This instrument is so constructed that it appears to throw the image of the object upon the white sheet of paper on the small table at the right hand where the drawing is made, at the same time that it enables the same eye to see the pencil and the right hand. In this way I made a careful drawing of *B. termo* and its flagellum, magnified 5000 diameters. Here is a projection of the drawing made. But I subsequently avoided paper, and used under the camera a most carefully prepared surface of ground glass. When the drawing was made I placed on the drawing a drop of Canada balsam, and covered it with a circle of thin glass, just like any other microscopic mounted object. This is a micro-slide so prepared. Now you can see that I only have to lay this on the stage of a microscope, make it an object for a low power, and use a screw micrometer to find how many flagella go to the making of a body. The result is given in the figure: you see that ten flagella would fill the area occupied by the diameter of the body.

In the case chosen the body was the 1/20400th of an inch wide, and therefore, when divided by ten, gave for the flagellum a thickness of the 1/204000th of an English inch. In the end I made fifty separate drawings with four separate lenses. I averaged the result in each fifty; and then took the average of the total of 200, and the mean value of the width of the flagellum was the 1/2047700th of an English inch. It will be seen, then, that we are possessed of instruments which, when competently used, will enable us to study the life-histories of the putrefactive organisms, although they are the minutest forms of life. I have stated that they were the inevitable accompaniments of putrescence and decay. You learned from a previous illustration the general appearance of the Bacteria: they are the earliest to appear whenever putrefaction shows itself. In fact, the pioneer is this—the ubiquitous *Bacterium termo*. The order of succession of the other forms is by no means certain. But whenever a high stage of decomposition is reached a group of forms represented by these three will swarm the fluid. These are the Monads, they are strictly putrefactive organisms, they are midway in size between the least and largest Bacteria, and are, from their form and other conditions, more amenable to research, and twelve years ago I resolved, with the highest power lenses and considerable practice in their use, to attack the problem of their origin; whether as physical products of the not-living, or as the natural progeny of parents.

But you will remember that only a minute drop of fluid containing them can be examined at one time. This minute drop has to be covered with a minute film of glass not more than the 200th of an inch thick. The highest lenses are employed, working so near as almost to touch the delicate cover. Clearly, then, the film of fluid would rapidly evaporate and cause the destruction of the object studied. To prevent this an arrangement was devised by which the lens and the covered fluid under examination were used in an air-tight chamber, the air of which was kept in a saturated condition; so that being like a saturated sponge unable to take in any more it left the film of fluid unaffected. But to make the work efficient I soon found that there must be a second observer. Observation by leaps was of no avail. To be accurate it must be unbroken. There must be no gap in a chain of demonstration. A thousand mishaps would occur in trying to follow a single organism through all the changes of successive hours to the end. But, however many failures, it was evident we must begin on another form at the earliest point again, and follow it to the close. I saw soon that every other method would have been merely empirical, a mere piecemeal of imagination and fact. When one observer's ability to continue a long observation was exhausted, there must be another at hand to take up the thread and continue it; and thus to the end. I was fortunate indeed at this time in securing the ready and enthusiastic aid of Dr. J. J. Drysdale, of Liverpool, who practically lived with me for the purpose and went side by side with me to the work. We admitted nothing which we had not both seen, and we succeeded each other consecutively, whenever needful, in following to the end the complete life-histories of six of these remarkable forms.

I will now give you the facts in relation to two which shall be typical. We obtained them in enormous abundance in a maceration of fish. I will not take them in the order of our researches, but shall find it best to examine the largest and the smallest. The appearance of the former is now before you. It is divergent from the common type when seen in its perfect condition, avoiding the oval form, but it resumes it in metamorphosis. It is comparatively huge in its proportions, its average extreme length being the 1,000th of an inch. Its normal form is rigidly adhered to as that of a rotifer or a crustacean. Its body-substance is a structureless sarcode. Its differentiations are a nucleus-like body, not common to the monads; generally a pair of dilating vacuoles, which open and close like the human eyelid, ten to twenty times in every minute; and lastly, the unusual number of four flagella. That the power of motion in these forms and in the Bacteria is dependent upon these flagella I believe there can be no reasonable doubt. In the monads, the versatility, rapidity, and power of movement are always correlated with the number of these. The one before us could sweep across the field with majestic slowness, or dart with lightning swiftness and a swallow's grace. It could gyrate in a spiral, or spin on its axis in a rectilinear path like a rifled bullet. It could dart up or down, and begin, arrest, or change its motion with a grace and power which at once astonish and entrance. Fixing on one of these monads then, we followed it doggedly by a never ceasing movement of a "mechanical stage," never for an instant losing it through all its wanderings and gyrations. We found that in the course of minutes, or of hours, the sharpness of its outline slowly vanish, its vacuoles disappear, and it lost its sharp caudal extremity, and was sluggishly amœboid. This condition intensified, the amœboid action quickened as here depicted, the agility of motion ceased, the nucleus body became strongly developed, and the whole sarcode was in a state of vivid and glittering action.

If now it be sharply and specially looked for it will be seen that the root of the flagella *splits*, dividing henceforth into two separate pairs. At the same moment a motion is set up which pulls the divided pairs asunder, making the interval of sarcode to grow constantly greater between them. During this time the nuclear body has commenced and continued a process of self-division; from this moment the organism grows rapidly rounder, the flagella swiftly diverge. A bean-like form is taken; the nucleus divides, and a constriction is suddenly developed; this deepens; the opposite position of the flagella ensues, the nearly divided forms now vigorously pull in opposite directions, the constriction is thus deepened and the tail formed. The fibre of sarcode, to which the constricted part has by tension been reduced, now snaps, and two organisms go free. It will have struck you that the new organism enters upon its career with only *two* flagella and the normal organism is possessed of four. But in a few minutes, three or four at most, the full complement were always there. How they were acquired it was the work of months to discover, but at last the mystery was solved. The newly-fissioned form darted irregularly and rapidly for a brief space, then fixed itself to the floor or to a rigid object by the ends of its flagella, and, with its body motionless, an intense vibratory action was set up along the entire length of these exquisite fibres. Rapidly the ends split, one half being in each fibre set free, and the other remaining fixed, and in 130 seconds each entire flagellum was divided into a perfect pair.

Now the amœboid state is a notable phenomenon throughout the monads as precursive of striking change. It appears to subserve the purpose of the more facile acquisition and digestion of food at a crisis. And this augmented the difficulty of discovering further change; and only persistent effort enabled us to discover that with comparative rareness there appeared a form in an amœboid state that was unique. It was a condition chiefly confined to the caudal end, the sarcode having become diffluent, hyaline, and intensely rapid in the protrusion and retraction of its substance, while the nuclear body becomes enormously enlarged. These never appear alone; forms in a like condition are diffused throughout the fluid, and may swim in this state for hours. Meanwhile, the diffluence causes a spreading and flattening of the sarcode, and swimming gives place to creeping, while the flagella violently lash. In this condition two forms meet by apparent accident, the protrusions touch, and instant fusion supervenes. In the course of a few seconds there is no disconnected sarcode visible, and in five to seven minutes the organism is a union of two of the organisms, the swimming being again resumed, the flagella acting in apparent concert. This may continue for a short time, when movement begins to flag and then ceases. Meanwhile, the bodies close together, and the eyenots or vacuoles melt together, the two nuclei become one and disappear, and in eighteen hours the entire body of "either has melted into other," and a motionless, and for a time irregular, sac is left. This now becomes smooth, spherical, and tight, being fixed and motionless. This is a typical process; but the mingled weariness and pleasure realised in following such a form without a break through all the varied changes into this condition is not easily expressed.

But now the utmost power of lenses, the most delicate adjustment of light, and the keenest powers of eyesight and attention must do the rest. Before the end of six hours the delicate glossy sac opens gently at one place, then there streams out a glairy fluid densely packed with semi-opaque granules, just fairly visible when their area was increased six millions of times, and this continued until the whole sac was empty and its entire contents diffused. To follow with our utmost powers these exquisite specks was an unspeakable pleasure, a group seen to roll from the sac, when nearly empty, were fixed and never left. They soon palpably changed by apparent swelling or growth, but were perfectly inactive; but at the end of three hours a beaked appearance was presented. Rapid growth set in, and at the end of another hour, how has entirely baffled us, they acquired flagella and swam freely; in thirty-five minutes more they possessed a nucleus and rapidly developed, until at the end of nine hours after emission a sporule was followed to the parent condition and left in the act of fission. In this way, with what difficulties I need not weary you, a complete life-cycle was made out.

And now I will invite your attention to the developmental history of the *most minute* of the six forms we studied. In form it is a long oval, it is without visible structure or differentiation within, and is possessed of only a single flagellum. Its utmost length is the 5000th of an inch. Its motion is continuous in a straight line, and not intensely rapid, nor greatly varied, being wholly wanting in curves and dartings. The copiousness of its increase was, even to our accustomed eyes, remarkable in the extreme, but the reason was discovered with comparative ease. Its fission was not a division into two but into many. The first indication of its approach in following this delicate form was the assumption rapidly of a rounder shape. Then followed an amœboid and uncertain form, with an increased intensity of action which lasted a few moments when lassitude supervened, then perfect stillness of the body, which is now globular in form, while the flagellum feebly lashed, and then fell upon and fused with the substance of the sarcode. And the result is a solid, flattened, homogeneous ball of living jelly.

To properly study this in its further changes, a power of from three to four thousand diameters must be used, and with this I know of few things in the whole range of minute beauty more beautiful than the effect of what is seen. In the perfectly motionless flattened sphere, without the shimmer of premonition and with inconceivable suddenness, a white cross smites itself, as it were, through the sarcode. Then another with equal suddenness at right angles, and while with admiration and amazement one for the first time is realising the shining radii, an invisible energy seizes the tiny speck, and fixing its centre, twists its entire circumference, and endows it with a turbined aspect. From that moment intense interior activity became manifest. Now the sarcode was, as it were, kneading its own substance, and again an inner whirling motion was visible, reminding one of the rush of water round the interior of a hollow sphere on its way to a jet or fountain. Deep fissures or indentions showed themselves all over the sphere; and then at the end of ten or more minutes all interior action ceased, and the sphere had segmented into a coiled mass. There was no trace of an investing membrane; the constituent parts were related to each other simply as the two separating parts of an ordinary fission; and they now commenced a quick, writhing motion like a knot of eels, and then, in the course of from seven to thirty minutes separated, and fully endowed with flagella swam freely away, minute but perfect forms, which by the rapid absorption of pabulum attained speedily to the parent size.

It is characteristic of this group of organic forms that multiplication by self-division is the common and continuous method of increase. The other and essential method was comparatively rare and always obscure. In this instance, on the first occasion the continuous observation of the same "field" for five days failed to disclose to us any other method of increase but this multiple-fission, and it was only the intense suggestiveness of past experience that kept us still alert and prevented us from inferring that it was the *only* method. But eventually we perceived that while this was the prevailing phenomenon, there were scattered amongst the others forms of the same monad *larger* than the rest, and with a singular granular aspect towards the flagellate end. It may be easily contrasted with the normal or ordinary form. Now by doggedly following one of these through all its wanderings a wholly new phase in the morphology of the creature was revealed. This roughened or granular form seized upon and fastened itself to a form in the ordinary condition. The two swam freely together, both flagella being in action, but it was shortly palpable that the larger one was absorbing the lesser. The flagellum of the smaller one at length moved slower, then sluggishly, then fell upon the sarcode, which rapidly diminished, while the bigger form expanded and became vividly active until the two bodies had actually fused into one. After this its activity diminished, in a few minutes the body became quite still, leaving only a feeble motion in the flagellum, which soon fell upon the body-substance and was lost. All that was left now was a still spheroidal glossy speck, tinted with a brownish yellow. A peculiarity of this monad is the extreme uncertainty of the length of time which may elapse before even the most delicate change in this sac is visible. Its absolute stillness may continue for ten or more hours. During this time it is absolutely inert; but at last the sac —for such it is—opens gently, and there is poured out a brownish glairy fluid. At first the stream is small, but at length its flow enlarges the rift in the cyst, and the cloudy volume of its contents rolls out, and the hyaline film that inclosed it is all that is left.

The nature of the outflow was like that produced by the pouring of strong spirit into water. But no power that we could employ was capable of detecting a *granule* in it. To our most delicate manipulation of light, our finest optical appliances, and our most riveted attention, it was a homogeneous fluid and nothing more. This for a while baffled and disturbed us. It lured us off the scent. We inferred that it might possibly be a fertilising fluid, and that we must look in other directions for the issue. But this was fruitless, and we were driven again to the old point, and having once more obtained the emitted fluid, determined to fix a lens magnifying 5000 diameters upon a clear space over which the fluid had rolled, and near to the exhausted sac, and ply our old trade of *watching*-unbroken observation.

The result was a reward indeed. At first the space was clear and white, but in the course of a hundred minutes there came suddenly into view the minutest conceivable specks. I can only compare the coming of these to the growth of the stars in a starless space upon the eye of an intense watcher in a summer twilight. You knew but a few minutes since a star was not visible there, and now there is no mistaking its pale beauty. It was so with these inexpressibly minute sporules; they were not there a short time since, but they grew large enough for our optical aids to reveal them and there they were. Such a field after one hour's watching I present to you. And here I would remark that these delicate specks were unlike any which we saw emerge directly from the sac as granules. In that condition they were always semi-opaque, but here they were transparent, and a brown yellow, the condition always sequent upon a certain measure of growth.

To follow these without the loss of an instant's vision was pleasure of the highest kind. In an hour and ten minutes from their first discovery they had grown to oval points. In one hour more the specks had become beaked and long. And this pointed end was universally the end from which the flagellum emerged. With the flagellum comes motion, and with that abundant pabulum, and therefore rapid growth. But when motion is attained we are compelled to abandon the mass and follow one in all its impetuous travels in its little world; and by doing so we are enabled to follow the developed speck into the parent condition and size, and not to leave it until it had, like its predecessors, entered on and completed its wonderful self-division by fission.

It becomes then clearly manifest that these organisms, lowly and little as they are, arise in fertilised parental products. There is no more caprice in their mode of origin, than in that of a crustacean or a bird. Their minuteness, enormous abundance, and universal distribution, is the explanation of their rapid and practically ubiquitous appearance in a germinating and adult condition. The presence of putrefiable or putrescent matter determines at once the germination of the always-present spore. But a new question arises. These spores are definite products. In the face of some experimental facts one was tempted to inquire, have these spores any capacity to resist heat greater than the adults? It was not easy to determine this question. But we at length were enabled to isolate the germs of seven separate forms, and by means of delicate apparatus, and some twelve months of research, to place each spore sac in an apparatus so constructed that it could be raised to successive temperatures, and without any change of conditions examined on the stage of the microscope.

In this way we reached successive temperatures higher and higher until the death point—the point beyond which no subsequent germination ever occurred—was reached in regard to *each* organism. The result was striking. The normal death point for the adult was 140° F. One of the monads emitted from its sac minute mobile specks—evidently living bodies—which rapidly grew. These we always destroyed at a temperature of 180° F. Three of the sacs emitted spores that germinated at every temperature under 250° F. Two more only had their power of germination destroyed at 260° F. And one, the least of all the monad forms, in a heat partially fluid and partially dry, at all points up to 300° F. But if wholly in fluid it was destroyed at the point of 290° F. The average being that the power of heat resistance in the spore was to that of the adult as 11 to 6. From this it is clear that we dare not infer spontaneous generation after heat until we know the life-history of the organism.

In proof of this I close with a practical case. A trenchant and resolute advocate of the origin of living forms *de novo*, has published what he considers a crucial illustration in support of his case. He took a strong infusion of common cress, placed it in a flask, boiled it, and, whilst boiling, hermetically sealed it. He then heated it up in a digester to 270° F. It was kept for nine weeks and then opened, and, in his own language, on microscopical examination of the earliest drop "there appeared more than a dozen very active monads." He has fortunately measured and roughly drawn these. A facsimile of his drawing is here. He says that they were possessed of a rapidly moving lash, and that there were other forms without tails, which he assumed were developmental stages of the form. This is nothing less than the monad whose life-history I gave you last. My drawings, magnified 2500 diams., of the active organism and the developing sac, are here.

Now this experimenter says that he took these monads and heated them to a temperature of about 140° F., and they were all absolutely killed. This is accurately our experience. But he says these monads arose in a closed flask, the fluid of which had been heated up to 270° F. Therefore, since they are killed at 140° F., and arose in a fluid after being heated to 270° F., they must have arisen *de novo!* But the truth is that this is the monad whose spore only loses its power to germinate at a temperature (in fluid) of 290°, that is to say, 20° F. higher than the heat to which, in this experiment, they had been subjected. And therefore the facts compel the deduction that these monads in the cress arose, not by a change of dead matter into living, but that they germinated naturally from the parental spore which the heat employed had been incompetent to injure. Then we conclude with a definite issue, viz., by experiment it is established that living forms do not now arise in dead matter. And by study of the forms themselves it is proved that, like all the more complex forms above them, they arise in parental products. The law is as ever, only that which is living can give origin to that which lives.

WHIRLWINDS AND WATERSPOUTS[1]

WHIRLWINDS, whether on sea or on land, have their characters in great part alike. For simplicity it will be convenient to begin by taking up only the case of whirlwinds on sea, as thus the necessity for alternative expressions to suit both cases, that of sea and that of land, will be avoided.

It may be accepted as a fact sufficiently established, both by dynamic theory and by barometric observations, that at the sea-level the pressure of the air is less in the neighbourhood of the axis of whirl than it is at places farther out from the axis, though within the region of the whirl. The apocentric force (centrifugal force) of the rapidly-revolving air resists the inward pulsive tendency of the greater outer than inner pressure. But close over the surface of the sea there exists necessarily a lamina of air greatly deadened as to the whirling motion by fluid friction, or resistance, against the surface of the sea; and all the more so because of that surface being ruffled into waves and often broken up into spray. This frictionally-deadened lamina exerts, because of its diminished whirl speed, less apocentric force than the quicker-revolving air above it, and so is incapable of resisting the inward pulsive tendency of the greater outer than inner pressure already mentioned. Hence, while rushing round in its whirl, the air of that lamina must also be flowing in centreward.

The influx of air so arriving at the central region cannot remain there continually accumulating; it is not annihilated, and it certainly does not escape downwards through the sea. There is no outlet for it except upwards, and as a rising central core it departs from that place. This is one way of thinking out some of the conditions of the complex set of actions under contemplation; but there is much more yet to be considered.

Hitherto, in the present paper, nothing has been said as to the cause or mode of origin of the diminished barometric pressure which, during the existence of the whirlwind, does actually exist in the central region. Often in writings on this subject the notion has been set forth that the diminished pressure is caused by the rapid gyratory motion of the whirling air; but, were we to accept that view, we would have still to ask, How does the remarkably rapid whirling motion receive its own origin? The reply must be that the view so offered is erroneous; and that, in general, a diminished pressure existing at some particular region is the cause rather than the effect of the rapid whirling motion; though in some respects indeed these two conditions can be regarded as being mutually causes and effects, each being essential to the maintenance of the other, while there are also some further promoting causes or conditions not as yet here mentioned.

It seems indubitably to be the truth that ordinarily for the genesis of a whirlwind the two chief promoting conditions are: firstly, a region of diminished barometric pressure, this diminution of pressure being, it may be presumed, due to rarefaction of the atmosphere over that region by heat, and sometimes, further, by its condition as to included watery vapour; and, secondly, a previously existing revolutional motion, or differential horizontal motion, of the surrounding air, such revolutional or differential motion being not necessarily of high velocity at any part.

The supposed accumulation of air rarefied by heat or otherwise, for producing the abatement of pressure may, the author supposes, in some cases extend upwards throughout the whole depth of the atmosphere; and in some cases may be in the form of a lower warm lamina which somehow may have been overflowed or covered by colder air above, through which, or into which, it will tend to ascend: or the lower lamina may in some cases be warmed in any of several ways, and so may get a tendency to rise up through the colder superincumbent atmosphere. On this part of the subject the author believes there is much scope for further researches and advancements both observational and considerational;—that is to say, by encouragement of a spirit towards accurate observation; and by collection and scrutiny of observed facts and appearances; and by careful theoretical consideration founded on observational results or suppositions.

To the author it seems probable that the great cyclones may have their region of rarefied air extending up quite to the top of the atmosphere; while often whirlwinds of smaller kinds, many of the little dust whirlwinds, for instance, which are frequently to be seen, may terminate, or gradually die out, at top in a layer or bed of the atmosphere different in its conditions, both as to temperature and as to original motion, from the lower layer in which the whirlwind has been generated. In many such cases the upper air may probably be cooler than the lower air in which the whirlwind originates.

On the subject of the actions going on at the upper parts or upper ends of whirlwind cores in most cases, the author feels that he is able to offer at present little more than suggestions and speculative conjectures. In very many descriptions of the appearances presented by those whirlwinds with visible revolving cores, which are called waterspouts, it is told that the first appearance of the

---

[1] Paper by James Thomson, LL.D., D.Sc., Professor of Civil Engineering and Mechanics in the University of Glasgow, read in Section A, at the British Association meeting at Montreal, on Monday, September 1.